electrons must be directed upon the nucleus. If they possess sufficient energy to penetrate the external levels of electrons in the mercury atom, they must reach the positively charged nucleus and be captured by it. Since the loss of an electron (as a β-ray) by the nucleus of an element in the radioactive disintegration of an element results in the atomic number of the element in question being increased by one, the gain of an electron by an atomic nucleus must result in the diminution of the atomic number by one. This is quite general. In the case of an isotope of mercury of atomic number 80, the product will be an isotope of gold of atomic number 79. Upon existing knowledge it is simply a question of (1) the potential sufficient to drive the electron through the outer levels of electrons surrounding the mercury nucleus until it comes within the sphere of attraction of the powerfully charged nucleus; (2) whether the exceedingly small fraction of direct collisions with the nucleus that is to be anticipated will be sufficient to enable the gold produced to be detected.

As regards the first, it may be expected that the repulsion of the external shell of mercury electrons will diminish rather than prevent altogether the chance of the radiant electron reaching the nucleus; for once the shell is penetrated, the resultant force on the radiant electron must be on the average an attraction. Hence I had arrived at the conclusion that, not the transmutation, but the chemical detection of the gold produced, would probably be the more formidable experimental difficulty.

Of course, I need express no opinion on the correctness of the experimental results recorded, which naturally will have to be very rigorously confirmed by further work. I merely wish to point out that no atomic disruption is necessarily involved, and that, so far from conflicting with existing knowledge, the result in a sense follows naturally from it, the only question being one of the sensitiveness of the experimental methods of detecting gold.

FREDERICK SODDY.

Oxford, August 9.

The Transmission of a New Plant Virus Disease by Insects.

IN a paper read to the Imperial Botanical Conference held in London this year, I have described a variegated condition of a number of Graminaceous plants, characterised by chlorosis of the leaves in narrow broken stripes parallel to the veins and a reduction in the power of growth of the plant. In maize it has been recognised for many years as a factor limiting production in the coastal and midland areas of Natal, and was described as long ago as 1901 by Fuller (First Report, Government Entomologist, Natal). Similar conditions are found in sugar cane and a number of other grasses.

I have given evidence in the paper mentioned for the belief that this variegated condition is a disease of a type similar to the now well-known mosaic disease of sugar cane, maize and other grasses, but is not identical with it. These conclusions were at that time based upon general observations and lacked evidence of experimental transfer. Recent work upon insect-transmission of the disease occurring in maize affords confirmation of the conclusions originally reached.

In a series of experiments I have been able to demonstrate the ability of the adults of a Jassid leaf-hopper, an undescribed species of the genus Balclutha, to produce the disease in healthy maize plants when transferred to them from diseased plants. Hoppers taken from healthy plants have failed to produce the disease. All plants have been protected from outside infection for the whole period of the experiment, and no control plants have developed the disease. The role of *Aphis Maidis* Fitch as the vector of mosaic disease between sugar cane, maize, and certain grasses has been established by several workers, and has been confirmed by me under South African conditions. All attempts to secure infections of this new disease through the agency of *A. Maidis* have failed.

The experiments referred to have been carried out with leaf-hoppers collected in diseased maize fields. A proportion only of such individuals are capable of producing the disease, although the maize may be almost universally infected. No individuals, however, which have been once proved to be vectors, have failed to transmit the disease to all plants to which they have been subsequently moved.

Preliminary attempts to secure transmission of the disease from maize to sugar cane and grasses have not succeeded. It must, therefore, remain in doubt whether it is a single disease which occurs in the different hosts, although field observations and similarity of symptoms would point to this conclusion.

H. H. STOREY.

The Natal Herbarium,
Durban, July 8.

Isotopes of Mercury and Bismuth and the Satellites of their Spectral Lines.

IN his letter to NATURE (May 31, p. 781), Prof. Runge criticised the result of measurements on the satellites of mercury and bismuth lines in connexion with the isotopes, which I and my co-workers have communicated to NATURE (March 29, p. 459), as not convincing. It gives me much pleasure that our note has attracted the attention of such an eminent spectroscopist and mathematician as Prof. Runge. The aim of that note was to show that the wave-length differences $\delta\lambda$, according to our formula, which involves the masses of different isotopes, are found among the satellites of mercury and bismuth lines. Prof. Runge discarded *i* and *j*, and took wave-length differences of observed satellites at random, which is contrary to our view, as regards the selection. The example given by Prof. Runge seems to me to be *lauter Zahlenspielerei*.

Perhaps the lines we have cited have too many satellites to incur a criticism; there are, however, many lines with a small number of satellites, and in which the number of selections is but limited. I can cite many such examples, but as the result obtained on satellites of twenty principal lines of mercury has already appeared in the *Japanese Journal of Physics*, vol. ii., pp. 121-162, the publication of which was much retarded by the disastrous earthquake, I consider it needless to occupy the columns of NATURE with the details. We have obtained more than 130 coincidences with our formula, within the limits of errors of observation, in mercury lines from the yellow to the ultra-violet region, so that the probability of its validity is very great, and cannot be considered as due to mere chance. The report on bismuth lines will shortly appear in the above Journal.

If we completely separate the isotopes, investigate the difference between the satellites and confirm our result, the proof will no more be open to criticism. Only partial separation of isotopes is at present possible, but even this will help us to study the intensity difference of the satellites, by which the present question can be in some degree settled more directly than by the arrangement of the satellites.

H. NAGAOKA.

Institute of Physical and Chemical Research,
Komagome, Tokyo, July 2.